

SYMPOSIUM

Breaking the Waves in Science Studies:

Comment on H.M. Collins and Robert Evans,
'The Third Wave of Science Studies'

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The human mind finds threesomes reassuring. Perhaps it has to do with the lateral symmetry of our bodies: right, left, and center. Three, at any rate, is a favorite structuring principle in art and argument. Triptych and trilogy. Thesis, antithesis, synthesis. Wide pendulum swings modulating to a restful mid-point.

To Harry Collins' credit, his threesomes have not always aimed for the comfort of the happy middle. In 1981, when he announced the three-stage 'empirical programme of relativism' (EPOR), Collins (1981) charted an ambitious research program for the then young field of science studies. The two initial stages were indeed formulated as thesis and antithesis: first, display the interpretive flexibility of experimental claims; then, show how interpretation solidifies and loses ambiguity, despite possibilities for endless debate. The program's third stage, however, was a call for expanding rather than limiting the analyst's field of vision, by relating the production of scientific knowledge to its wider social and political contexts. Among other things, this invitation to science studies scholars to step outside the self-appointed boundaries of science and technology opened the door to productive conversations between the sociology of scientific knowledge and neighboring social science fields, such as history, anthropology, and political science.

In this 60-page discussion paper, written more than two decades later, Collins and his co-author Robert Evans (hereafter C&E) try once again to fit science studies within a three-fold schema (Collins & Evans, 2002). The 'stages' now have become 'waves', but the authors' object, as in Collins' earlier programmatic piece, is to position existing work within the first two waves and set the stage for a third one. Unlike the third stage of EPO,

however, the proposed Wave Three does not prefigure a leap toward a more ambitious research agenda. It seems very much a drawing in of the horns. The authors' primary concern is expertise. Let us not, C&E enjoin us, throw out the baby of true expertise with the bath water of politically correct participation. They state the problem this way: 'Should the political legitimacy of technical decisions in the public domain be maximized by referring them to the widest democratic process, or should such decisions be based on the best expert advice?' (235).

Thesis, antithesis, synthesis. The new 'wave theory' of science studies C&E propose very much has this character. The waves are seen as chronological, and science studies is periodized by these means with regard to one particular issue: the nature of expertise and the principles for public engagement in decision-making involving experts. Wave One, we are told, was the era of positivism, in which expertise was taken for granted and hence quite unproblematic. Science was thought to be esoteric and authoritative, and, in a corollary later found unacceptable, any person who embodied 'good science' was permitted to hold forth unchallenged as an expert in his (rarely her) field of knowledge. Wave Two, by contrast, was the era of unbridled social constructivism, in which relativism about the foundations of expert knowledge reigned supreme; this led, C&E suggest, to the equally unstable conclusion that all technical decision-making should be wide open to public participation. With Wave Three, we now learn, retrenchment is in order. We need to recognize that there is legitimate expertise on technical issues; not everybody can or should hope to participate in all aspects of technical deliberation; and the demarcation between science and politics, so enthusiastically dissolved by Wave Two scholars, should be at least partially reinstated.

The bottom line advocated by C&E, then, is that there can be too much as well as too little public involvement in technical decision-making, and we need better conceptual tools with which to determine how much is 'just right' in any given situation. To further this argument, the authors introduce an array of new terms, with several rather abstruse diagrams to supplement them. Expertise is divided into three types: 'interactional' (sufficient to converse with experts in a given field), 'contributory' (sufficient to contribute to the field itself), and 'referred' (sufficient to understand what it means to contribute to a field). Experts are seen as needing two additional faculties: 'translation' (ability to move between different social worlds) and 'discrimination' (ability to make distinctions between different kinds of claims and sources of credibility). Finally, C&E divide up science itself into four types (normal, Golem,¹ historical, and reflexive historical), each requiring a different approach to decision-making. These categories of expertise, expert faculties, and science are intended to help people (decision-makers?) draw the line between appropriate and inappropriate inclusiveness in technical debates conducted in public domains.

This is a hugely complex architecture for an issue that is far older than the concerns of contemporary science studies (the law, for instance, has

been grappling with its institutional problems of expertise since the late 18th century; see Jasanoff, 1995: 42–68). Before embracing C&E's framework too eagerly, therefore, one must ask whether the foundations on which it is built are entirely secure. Does C&E's approach to the question of expertise make sense, and are the categories in which they would have us think about it sensible? A closer look induces skepticism. C&E's history, on examination, looks specious, as overly neat periodizations have a way of doing. There is as well a reductionist quality to their analysis that sits uneasily with the complex dynamics of expertise in modern societies, and the richness of writing about it. The difficulties cluster under (dare I say it?) three headings: (1) a misleading characterization of the relevant science studies literature; (2) a misconception of the foundations of expertise in the public domain; and (3) a misunderstanding of the purposes of public participation in contemporary democratic societies. Let us consider each.

A Depleted Intellectual History

Second Wave work in science studies, C&E assert, was geared mainly toward taking scientific and technical knowledge down a few pegs, showing how extra-scientific factors enter into knowledge-making and the closure of disputes, and thereby establishing the relativist point that there is nothing so very special after all about scientific knowledge. C&E see work in this vein as having tried to put 'lay expertise' and specialist expertise on an equal footing, an effort they evidently consider counterproductive. In a lengthy critique of Brian Wynne's familiar case study of the Cumbrian radiation controversy following Chernobyl, C&E reinterpret Wynne's example as involving miscommunication between the certified expertise of the nuclear experts and the non-certified expertise of the sheep farmers. The farmers, according to C&E, lacked the interactional expertise needed to bring their knowledge into fruitful dialogue with the knowledge of radiation experts. This was not an instance of failed public participation, they say, so much as a failed attempt to integrate policy-relevant knowledge from multiple sources governed by different certification regimes.

The specifics of this argument are dealt with by Wynne himself elsewhere in the present issue of *Social Studies of Science* (Wynne, 2003). What concerns me here is the persistently reductive reading of sources that the argument exemplifies. For more than a generation now, work that C&E rather casually group together as 'Wave Two' has formed part of a project of social theorizing that goes far beyond the relativism which the authors focus on. To say that work as diverse as that of Brian Wynne, Michel Callon, Bruno Latour, and for that matter my own (compare with C&E, 2002: 276–77), represents Wave Two's preoccupation with relativism is to seriously misread this work.

To be sure, for many years the sociology of scientific knowledge has been engaged in exploring the social underpinnings of expertise, but interesting work in this genre has concerned itself as much with delineating cognitively salient forms of life as with the knowledges that such forms of

life produce and sustain. It is this preoccupation with the mutual embedding of natural knowledge and social order, their co-production, in a word, which gives canonical work in science studies its staying power. Key to Wynne's Chernobyl case study in this regard is not merely that farmers and radiation experts possessed different, complementary knowledges about local soils, grazing conditions, and radioactive cesium uptake into vegetation. This is certainly a piece of Wynne's story, but more significant is the fact that these discrepancies were rooted in different life worlds, entailing altogether different perceptions of uncertainty, predictability and control. The knowledges stemming from these divergent experiential contexts were not simply additive; they represented radically 'other' ways of understanding the world.

C&E's account also draws artificial boundaries around what constitutes work relevant to expertise in science studies. Three well-known examples from the science studies literature will help reinforce the point I am making. In *Leviathan and the Air-Pump*, Steven Shapin and Simon Schaffer (Shapin & Schaffer, 1985: 332) discussed the interactive nature of scientific and political practice in the early modern period, stressing the multiple ways in which the two reinforced each other's credibility. In *Descent of Icarus: Science and the Transformation of Contemporary Democracy*, Yaron Ezrahi (1990) built on these insights to illuminate the ideological ties between modern science and liberal democracy, arguing that (not coincidentally) the rise of both depended on the existence of an enlightenment culture of public witnessing. In *Science in Action* (Latour, 1987) and many related works, Bruno Latour stressed the materiality of the work required to wrest 'facts' from nature and to make them tractable as scientific knowledge.

All three books have played key roles in moving our understanding of scientific knowledge-making away from the simple positivism of the Wave One model of C&E. All three display the irreducible sociality of science. All are essential reading if one is to make sense of the institutional authority of science in the modern world, an authority that is continually called upon to legitimate assertions of technical expertise. Yet, none of the three merely deconstructs or relativizes science as postulated for Wave Two; to the contrary, all may be seen as explicating, in various ways, the very well-springs of science's power in society. Most important, not one of these authors essentializes the nature of expertise, showing it instead to be always contingent, historically situated, and grounded in practice. All this is a lot closer to the third stage of EPOR as imagined by Collins (1981) than to C&E's Wave Two as adumbrated in 2002.

Why do C&E not cite these books, among many possible others, as bearing on the problem of expertise? How would books such as these have affected their 'wave theory' if they had been taken on board? The absences in C&E's intellectual history I think are telling. There is an important point to be made here about the ways in which we, as a field, read and represent our own collective work. In a review some years ago of Noretta Koertge's (1998) edited contribution to the 'science wars', I took issue, as follows,

with the practices of reading displayed by the science warriors of the 1990s:

A more troubling problem is the quality of reading evident in many of these essays. Fixated on the truth-value of specific works, the critics display little awareness of how those works figure within the overall tapestry of science studies. These thoroughly decontextualized readings take no account of how given works have been received within the field, let alone of their authors' overall significance and impact. . . Ironically, this lack of discrimination – the disregard of standards that animate the field's professional practitioners – is precisely what philosophically inclined critics, such as Michael Ruse in this volume, most deplore about the accounts of science offered by science studies. (Jasanoff, 1999: 497)

It would be poetic justice indeed if we ourselves fell victim to similar habits of stochastic reading. (Although my comments in this piece were prompted by C&E's discussion paper, many of my reservations about the quality of reading in our field apply equally well to the subsequent paper in the same issue of *Social Studies of Science*, which deals with science studies and activism, and is based on similarly reductionist and unreflexive readings of the contributions of the field [Woodhouse et al., 2002]. But that is a problem for another day.)

One must be fair. C&E's ambitious project required them to distill masses of work, and the helpful 11-page Appendix goes into far more detail than the preceding text does about the intellectual basis for their analysis. In particular, references to my own work, firmly relegated to 'Wave Two', lead off a section entitled 'The Nature of Expertise'. I wish I could say I recognize myself in these paragraphs. This is not the shock of non-recognition that comes from seeing hidden aspects of ourselves through others' less normalized eyes, as when our 'true' selves are revealed in a photograph rather than a mirror (showing that our bodies are not so laterally symmetrical after all!). Nor is it a synecdoche, a rhetorically breathtaking citation of a part for the whole. No, this is something different. This is non-recognition of a kind that comes from seeing a few isolated statements and references privileged over the thrust of an entire corpus of writing, so that the work as a whole seems denuded of its central meaning.

How then would I represent what I say about expertise? Well, to begin with, I have always insisted that expertise is not merely something that is in the heads and hands of skilled persons, constituted through their deep familiarity with the problem in question, but rather that it is something acquired, and deployed, within particular historical, political, and cultural contexts. Expertise relevant to public decisions, I have further shown, responds to specific institutional imperatives that vary within and between nation states. Accordingly, who counts as an expert (and what counts as expertise) in UK environmental or public health controversies may not necessarily be who (or what) would count for the same purpose in Germany or India or the USA. Different bodies of expert knowledge come

into being, with their associated markers of excellence and credibility, through disparate contingencies of politics and knowledge production in national decision-making settings. In the past 20 years, for example, quantitative risk assessment has gained a reputation for objectivity in the USA that it does not enjoy to anything like the same degree in European regulatory circles. Finally, what operates as credible expertise in any society corresponds to its distinctive civic epistemology: the criteria by which members of that society systematically evaluate the validity of public knowledge. In a litigious society such as that in the USA, expectations of openness, transparency, and the right to look behind formal claims are deeply engrained in a multiplicity of institutional practices. These grounded expectations influence not only the processes by which the public relates to experts, but also the content and discourses of expertise. Another society, with different traditions for producing and testing public knowledge, might dress up its expertise in other guises.²

None of this erases or invalidates the existence of boundaries between experts and others in society, as C&E's Figure 7 (250) asserts Wave Two work has done. Rather, it invites us to take the boundaries themselves as entry points for inquiry into the relations between science and power, to ask how they come about, and what functions they serve in channeling both knowledge and politics. The questions that loom as interesting, then, have to do with: the nature of categories and classifications (as in the influential work of Michel Foucault and Ian Hacking); with the agents, instruments and processes that produce these classifications; with patterns of inclusion and exclusion on either side of the line of expertise; and with the influence of history and culture on the drawing and redrawing of these kinds of boundaries. The project of looking at the place of expertise in the public domain appears in this light as a project in political (more particularly democratic) theory, with epistemological questions embedded in it, but not wholly reducible to epistemology.

All this makes the kinds of distinctions that C&E try to draw between the scientific and political phases of decision-making (262, 276) seem at best naive and at worst misguided. To label some aspects of society's responses to uncertainty 'political' and some others 'scientific' makes little sense when the very contours of what is certain or uncertain in policy domains get established through intense and intimate science-society negotiations. There is, too, a forced linearity in the authors' suggestion that, once the politics of a situation have been properly sorted out, the 'real' science can proceed, independent of further political influence. Insulation of this kind is simply unthinkable if, wherever one cuts into decision-making processes, one finds a hybridization of science and politics as these terms are conventionally understood. Nor is there an objective Archimedean point from which an all-seeing agent can determine who belongs, and who does not, within the magic ring of expertise. I will return to these points later in the sections on institutions and on participation and its purposes.

Softness at the 'Core'

A great deal of the discussion paper's argument turns on C&E's apparent determination to resurrect Collins' idea of the 'core-set' in laboratory-based science controversies and to give it new meaning in the context of public decision-making. That translation unfortunately does not work. In technically grounded controversies in the policy domain, the central question most often is what is going to count as relevant knowledge in the first place; who possesses the right sorts of knowledge is secondary. 'Core-sets' may sometimes form (in decades of research I have observed such groups forming, for example, around 'dioxin science' and cancer-risk assessment), but these bodies of mutual accreditation and acknowledgment are very often the result of protracted controversy. Just as in sociology of science, 'facts' are depicted as the results (not the causes) of closure of controversy, so 'core-sets' in public domain technical controversies are the results, not the causes, of controversies settling into normalized patterns of debate. Importantly, it is when debates finally lose their public significance that one often sees these exclusive knowledge clubs emerging; we have a recognized cadre of 'dioxin scientists' today for just the reason that no one cares much any more about the proliferation of dioxins in the environment. That battle has been fought, and largely won, by those inclined to precaution with environmental toxins.

If we regard the very formation of expert 'core-sets' as a political phenomenon, then attention inevitably has to focus on the processes by which such sets are created, maintained, patrolled, and protected. In many areas of public policy, we may not be interested in re-examining the foundations of settled expertise in this way, but when controversy erupts, it becomes important to ask what sustains the authority of a particular group of experts and their expertise. Seemingly quiet areas can suddenly get destabilized and necessitate this sort of inquiry: for example, established forensic techniques such as: fingerprinting in the wake of the US Supreme Court's decision in *Daubert v. Merrell Dow Pharmaceuticals, Inc.*;³ experimental gene therapy protocols after 18-year-old Jesse Gelsinger's death in a University of Pennsylvania hospital in September 1999; and NASA's fault tree analysis after the Columbia shuttle's deadly break-up on re-entry in February 2003. In all these cases, something went wrong with the way policy processes had actively shaped the constitution (not merely the nature) of relevant expertise. The problem that science studies can help illuminate is how some crucially important knowledges, practices and norms were bounded out of decision-making that later turned out to be mistaken or tragic.

Understanding these processes of erasure has been a fundamental project of much science studies writing about expertise that C&E so unceremoniously consign to Wave Two. All-important here is the role of institutions, those profoundly efficient ordering agencies without whose involvement modernity as we know it would not exist. Curiously, institutions and their role in shaping societal expectations about expertise get

little attention in C&E's exploration. Yet, without taking institutional constraints into account, we can get very little mileage from notions such as a 'core-set' of expertise in the public domain. An example that I have discussed in detail elsewhere (Jasanoff, 1998, 2002), the case of silicone gel breast implant litigation in the USA, will illustrate the point.

When the first trickle of personal injury lawsuits began around silicone gel implants, there was little information in the scientific literature about the possible impact of these devices on women's health. One reason was that the devices had entered into commerce before federal legislation controlling medical devices was adopted; they were, in short, grandfathered in during a permissive pre-regulatory era. The litigation crystallized both the thing being studied and the production of knowledge about it. Specifically, the women plaintiffs represented in what became one of the largest mass tort actions ever to take place in the USA claimed that they were suffering from a form of immune system dysfunction that they named atypical connective tissue disease. It fell to the courts to decide whether this disease was real, whether it was caused by the implants, and whether the women were entitled to compensation. Needless to say, all these judgments required the courts to identify experts who could aid them in 'finding' the appropriate 'facts'.

It is hard to see how C&E's conceptual system would have helped the courts to find the right experts when they began their task. There was no cognizable body of 'breast implant science' at this stage in the proceedings. Agnosticism was the only responsible position to start with. Gradually, as the cases grew in volume and salience, some witnesses took on the character of acknowledged experts, simply by appearing in more than one lawsuit and thus being accredited by the performative rituals of the law. The nature of claimed expertise changed as the first epidemiological results began to be published (showing no causal connection between implants and typical connective tissue disease); it changed again when a federal court consolidated thousands of claims into a single, enormous case of multidistrict litigation and appointed a scientific panel to advise it.

It is important to stress, however, that this was not simply a case of knowledge developing and getting stronger through the inevitable forward march of research.⁴ The accumulation of epidemiological data, and their certification as the most authoritative scientific information available in these cases, accompanied changes in the representation of the plaintiffs and their complaints. Women claiming injury from the implants lost their status as suffering individuals and became anonymous 'statistical victims', whose bodies as such no longer figured in the settlement of claims. Their identity transformation was coupled to temporally congruent changes in the US courts, which, I have argued, were under increasing pressure at this turn of the century to modernize, become more sensitive to economic concerns, and standardize both their disposition of cases and their knowledge-making routines. Litigation, in other words, did not simply sort out 'good science' from 'bad'. The legal process determined which kinds of

bodies and framing, of people and of knowledges, would be entitled to societal respect and economic valuation. Where in this complex history would we locate the emergence of politically neutral expertise? Expertise did emerge, but with respect to the allocation of power it was neither neutral nor innocent.

Participatory Politics

I come now to the third strand of my critique of C&E, namely, their exceedingly narrow formulation of the purposes of public participation in technically grounded decision-making. In one sense, they are trying to lock the barn door after the horse has already bolted. The worldwide movement in legislation and public policy these days is toward, not away from, wider participation. The recent European Union White Paper on Governance, with its associated working group report on 'Democratising Expertise',⁵ offers but one example of this trend. In general, Western states have accepted the notion that democratic publics are adult enough to determine how intensely and in what manner they wish to engage with decision-making, subject only to the constraints of time and other resources. In US regulatory decision-making, for example, all federal agencies are required by law to engage the public at least by offering notice of their proposed rules and seeking comment. It is understood that any 'interested and affected' party has a right to participate in such processes. If this is the state of the world, then why should we pay attention to work that seems on its face to be looking for principles with which to limit the scope of public participation?

There are several possible answers to the question. What states have given, states can in principle also take away, and robust intellectual defenses against policy elitism are therefore always in order. There are, too, new arenas of technical decision-making opening up at the global level, where the rules of participation are not yet laid down and the pulls of democracy on behalf of a global civil society are tenuous at best. There are also worrying signs that some institutions that historically gave preference to lay concerns may be tightening up their entry requirements for ordinary citizens. The most prominent example is that of the US courts following the previously cited *Daubert* decision.

Contrary to C&E's apparent assumption, the most powerful argument for wider lay participation in expert decision-making is not that the public possesses some mysterious reservoir of lay expertise that is equal to the knowledge of scientists. There are several more compelling reasons that ought to be noted. First, it is worth remembering that the presumption in democratic societies is that all decisions should be as far as possible public; it is the exceptions that require justification. The American Freedom of Information Act, a crowning achievement of law in the Vietnam era, reflects just this sensibility. Second, public engagement is needed in order to test and contest the framing of the issues that experts are asked to

resolve. Without such critical supervision, experts have often found themselves offering irrelevant advice on wrong or misguided questions. Third, as we have seen, expertise is constituted within institutions, and powerful institutions can perpetuate unjust and unfounded ways of looking at the world unless they are continually put before the gaze of laypersons who will declare when the emperor has no clothes. Fourth, participation is an instrument for holding expertise to cultural standards for establishing reliable public knowledge, standards that constitute a culture's distinctive civic epistemology. Finally, and this is a point that C&E do seem to recognize at some level, participation can serve to disseminate closely held expertise more broadly, producing enhanced civic capacity and deeper, more reflective responses to modernity.

Returning to C&E's early formulation of the problem, we thus see that they have set up a false dichotomy. They asked: 'Should the political legitimacy of technical decisions in the public domain be maximized by referring them to the widest democratic process, or should such decisions be based on the best expert advice?' (Collins & Evans, 2002: 235). The issue, however, cannot be formulated in these simple either-or terms. We need both strong democracy and good expertise to manage the demands of modernity, and we need them continuously. The question is how to integrate the two in disparate contexts so as to achieve a humane and reasoned balance between power and knowledge, between deliberation and analysis. C&E's framework offers little satisfaction in finding workable answers to this pressing question.

Conclusion

On the perennial challenge of democracy in the face of specialization, C&E offer two sets of reflections: one is to define and classify expertise more accurately, especially in relation to its counterpart of 'lay' knowledge and abilities; the other is to prescribe for the field of science studies how it should organize future researches into the problem of expertise, under the banner of Wave Three. I have suggested in this comment that, while the challenge these authors state is undoubtedly important, their responses are significantly flawed. They are founded on a misreading of the science studies corpus bearing on the production of expert authority, a reluctance to take on board the dynamic, politically constituted nature of expertise, and a reductionist conception of the role of ordinary citizens in even the most esoteric domains of public decision-making. When we correct for these failures of vision, what emerges as most deserving of analysis by our field is how particular claims and attributions of expertise come into being and are sustained, and what the implications are for truth and justice; the intellectually gripping problem is not how to demarcate expert from lay knowledge or science from politics (though reflexive attempts to make such demarcations should be taken seriously). Such demarcations will keep being produced in any case, in the everyday work of scientists, citizens and institutions of governance. Showing what is at stake in the making of such

boundaries is another matter. That is a fitting place for critical science studies scholarship.

The metaphor of waves is powerful, exerting a pull on the human imagination as waves themselves do on bodies in the physical world. It is the more instructive, then, that C&E have chosen to represent their waves through tightly circumscribed circle diagrams that take no account of the inevitable, unstoppable force of moon-drawn oceans. Fortunately, the long waves of scholarship are no more susceptible to containment than the waves that break in long lines on our earthly shores. King Canute had his 'wave theory' right; Collins and Evans should consider his example.

Notes

I would like to thank Stefan Sperling and David Winickoff for extremely helpful comments on an earlier draft. Any errors or infelicities are of course my own.

1. The self-referential category of 'Golem science' is defined as: 'science which has the potential to become normal science, but has not yet reached closure to the satisfaction of the core-set' (Collins & Evans, 2002: 268). The term 'Golem' is borrowed from the title of Collins & Pinch (1998).
2. These ideas have been developed in a long series of writings about science, law, policy and comparative politics. Particularly important for this comment are the articles by Jasanoff (1986, 1987, 1990, 1992, 1995, 2002).
3. *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 509 US 579 (1993) (evidence ruling setting new criteria for the admissibility of scientific and technical evidence); *United States v. Llera Plaza*, 188 F. Supp. 2d 549 (E. D. Pa. 2002), vacating 179 F. Supp. 2d 492 (E. D. Pa. 2002) (federal district court decisions first invalidating and then reinstating expert testimony on matching fingerprints). For a history of fingerprinting, see Cole (2001).
4. A contrary view uninformed by science studies (reflecting C&E's Wave One, but also perhaps their Wave Three) may be found in Angell (1996).
5. European Commission, *Enhancing Democracy: A White Paper on Governance in the European Union*, Brussels, 25 July, 2001 (http://europa.eu.int/comm/governance/index_en.htm); also, *Report of the Working Group 'Democratising Expertise and Establishing Scientific Reference Systems'*, Brussels, May 2001 (http://europa.eu.int/comm/governance/areas/group2/report_en.pdf).

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